JPL Technical Memorandum 33-280

19. SOME ASPECTS OF BREAKDOWN AND CORONA PROBLEMS IN THE CRITICAL-PRESSURE RANGE

L. J. Frisco and W. T. Starr
Advanced Technology Laboratories
General Electric Company
Schenectady, New York

I would like to make a few comments relative to the breakdown problem in the critical-pressure range, and then I would like to take a few minutes to describe a program that we are currently working on at our Research and Development Center in Schenectady involving the performance of thin-wall hookup wire in the environments that might be experienced in the Apollo mission.

From the remarks and the discussion that I've heard both yesterday and today, I think that this group should be aware that such a program is underway and that it will soon (at least the first phase of it) be completed and that a rather comprehensive report will become available. I'm sure many of you are going to want to see those results.

Figure 19-1 substantiates something Earle Bunker said yesterday. He said the Paschen law or Paschen curve or Paschen minimum (terms that are sometimes not used too precisely) holds for a uniform field configuration but that with nonuniform fields we would not expect to get the same result.

Figure 19-1 shows some data taken from a paper by Wendell Starr in which he shows the comparison of the uniform field breakdown with what you would get with needle points as you do down through the critical pressure range. Of course, the needle points are much lower in the high-pressure range, and down around the minimum you actually get a phenomenon involving certain grading of the field and space charge and so forth, which can actually give you some higher voltages; then as you come up on the left-hand side of the Paschen curve the needle points fail to come back up again, and then when you get to even lower pressures (where emission takes over), the points do not behave well at all.

Mr. Bunker is quite right. You have to be careful in employing these principles so as to be aware of the field configurations you're dealing with.

Wendell Starr did rather a good job in analyzing the data that was available back in 1962, and he wrote a paper in two parts called "High Altitude Flashover and

Corona Problems," published in the May and June 1962 issues of Electrotechnology. There's no point of my going into many of the details of the paper; they have already, for the most part, been discussed here.

Now, unfortunately Mr. Starr couldn't be at this meeting. He's really our corona expert, and I'm sure he would have more to say about measurement techniques and so forth. He did tell me about a little experiment he ran that is described in Fig. 19-2, pointing up one of the anomalies in this critical-pressure region.

Figure 19-2 is a schematic, or functional diagram, of a hookup wire sample, and although it's shown as a solid conductor here, it was actually a No. 20 stranded conductor, insulated with silicone rubber about 40 mils. A conductive coating was applied to the outside of the insulation and the configuration was brought through the walls of the vacuum chamber. Then dc corona measurements were taken.

This wire had to be essentially free of corona at 2 kv dc, and it passed that test very easily. But when the pressure was reduced in the vacuum chamber, corona spikes occurred at about 1 kv at a chamber pressure of about 1/2 mm of Hg, and then they would disappear and again recur at about 5 mm when the pressure was being increased. Something was going on presumably in the region between the conductor and the insulation to give rise to corona spikes. Calculations were made, but it was concluded that it wasn't just the expansion due to the pressure of the gas in the wire. This explanation didn't seem to check out. As so often happens, just about the time that we were really getting involved with this problem, the specs were changed, resulting in the test being dropped. We never got back to the problem. This is the sort of unexpected problem that one can run into; it's another example of critical-region breakdown problems.

In some work I did at the Dielectrics Laboratory at Johns Hopkins a year or so ago (I'm a relative newcomer with the General Electric Company), we were making flashover measurements on various solid insulations in high vacuum. We found that even with the relatively low-loss materials one could get unexpected low values of flashover because dielectric heating caused localized, relatively high-pressure areas close to the surface of the specimen.

Here, again, I think we're getting into another type of breakdown in this critical PD range. The sample was a relatively small piece (1/4 x 2 3/4 in. and 1/8-in. thick) in a relatively large vacuum chamber; so when we talk about pressures, we have to be concerned with very localized pressures. It doesn't take much heating to drive off adsorbed gases and to cause degassing from the solid material itself. In listening to some of the arguments this morning about the use of foams (particularly in the RF range), I wonder how much off gassing or outgassing is being induced by RF heating of the relatively "lossy" dielectrics that are being used. Maybe you're helping to keep the foam pumped up because each time you give it a burst of RF you're generating some gas.

The materials that have been described are relatively lossy materials, if you consider just the solid part of the structure. In the experiments in which we were making flashover measurements, we also made measurements in the RF range and at that range the thermal conditions are so severe (because the specimen is well insulated thermally, being in a high vacuum) that even the very low-loss materials would exhibit a thermal type of breakdown before we got flashover in most cases.

The program we're currently working on is oriented toward the Apollo mission and so is concerned with the behavior of thin-wall hookup wire in the 5-psi oxygen atmosphere. Although we have been talking about one critical pressure range, I think there's another critical pressure in terms of the trouble it can cause: the 5-psi pure oxygen with moisture. Of course, we're also concerned about the operation in vacuum since a good part of the wire is on the outside of the spacecraft.

What we're doing is evaluating sixteen different kinds of thin-wall hookup wires. We're working with only one wire size or conductor size (No. 20 stranded wire), and here by thin wall I mean that the insulation is 10 mils or less.

The various types of construction, or the ones you would most expect, are ML coated TFE and FEP, plain type-E teflon, various H-film constructions, so-called LEM wires, and Martin wire. Some of these have a FEP dispersion on the outside; others have a TFE dispersion. Several of them have no dispersion on the outside.

There are also three different silicone rubber constructions (a plain silicone rubber, a kynar-jacketed silicone, and an H-film kynar-wrapped silicone -- a very unusual material) and two of the irradiated modified polyolefin wires (a kynar-jacketed wire and plain IMP). I'm not supposed to mention manufacturers' names here and I'm sure that none of you know where the latter one is made; the contract number is NAS 94549 and is sponsored by the NASA Manned Spacecraft Center in Houston.

I have no intention of going into detailed results of the test we're currently running as it would take far more time than we have, and I'm not in a position to discuss these results. But I think it is worth just a few minutes to give you a general description of the program.

The first thing we do with wire that is received is to put it through an insulation resistance test in which we immerse it in water for three days. A disturbing feature is that so much of the hookup wire (even though we only ordered 1,000 ft) is coming in very small lengths, an obvious indication that even the manufacturer can't make it in long lengths (or else he's giving us his odds and ends). Even with short lengths that are apparently hand picked, very many samples are failing this initial immersing test. This gives some cause for concern: is the spec too high? or is it unrealistic? or just what is the problem? If it passes the insulation resistance test, we give it a voltage withstand test (another immersion test) at 1,600 v. Then we have an insulation resistance test after exposure to 15-psi pure oxygen at a 100% relative humidity for 15 days. This latter test is made on a cabled type of specimen (six wires wrapped around one central wire) in which the resistance between the central wire and the six outer wires is measured.

Using the same sample, we also make corona start voltage measurements and corona extinction voltage measurements; these measurements are made both at 15 and 5 psi in pure oxygen with moisture. We then measure voltage breakdown.

All the voltage breakdown measurements are made on twisted pairs (usually NEMA twisted pairs). This test doesn't really give worthwhile engineering information because the voltages are far greater than the wires would ever experience in service, but it does serve as a tool for detecting degradation during other aging tests.

Flashover testing consists of applying a voltage to the stripped end of the wire. About 1 in. of the insulation is stripped off, and then fine bare conductor (approximately 1/4-in. long) is wrapped back on the insulation. A flashover is produced between this bare wire and the end of the conductor (in an atmosphere of pure oxygen at 5 psi, wet) in order to see what effect it has on the insulation. We're interested in whether we get charring, tracking, ignition, or what have you.

We have various other tests on the physical dimensions such as outside diameter, concentricity, conductor dimension, weight per thousand ft, and stripability. Stripability, of course, is quite another problem, and we're finding that although many of these thin-wall insulations are not difficult to strip, the difficulty is in not damaging the insulation left on the wire. In some of the breakdown tests we got some odd results that resulted from damage to insulation during the stripping operation.

Other problems include color durability, marking legibility, and compatibility with potting compounds. This latter one has led to some extremely interesting results. Here we're potting two types of samples. We age the potted twisted pair for 15 days at 15 psi of pure oxygen at 150°C, take it out of the aging, put in in water for 3 days, and then test it; and even though we're testing it in air, we've gotten fires in some cases and some pretty poor behavior in many others.

We don't have all the data yet. Although the program is only 3 months old, we already have a 259-page report on the first 8 weeks of it; we're about to finish it up in another two weeks, and much of the data is pouring in as I stand here talking. We've got about 15 people working on it, under the direction of Ken Mathes and myself.

The other type of compatibility with potting compound specimen is a pull-out type of mechanical specimen with which you're probably familiar; again, it goes through the same aging process, and then we pull it apart.

We test flexibility by using a repeated flex test, and we could stand here and talk all day about that. We don't like it too much. I think what we're really testing here are conductors rather than the insulation. We also have a cold flex, a repeated bent mandrel in which we wind from one mandrel to another mandrel and back again, and we do this at room temperature in liquid nitrogen at -196°C.

We have found in previous programs that testing in liquid nitrogen is a very sensitive means of detecting degradation. Here, again, we're not interested primarily in the behavior of the material in the liquid nitrogen because it may never see that environment. But we do find that in using this test degradation can be detected much sooner than at room temperature tests (although ultimately it will show up). It is a means of accelerating certain kinds of aging tests, and we use this as an end point in some of the aging tests where we're exposing the samples to oxygen temperature, moisture, ultraviolet, and x-ray radiation.

We also measure abrasion. The particular test we happen to be using is the NEMA repeated scrape abrasion (another subject we could talk about all day).

Blocking, that is, the wires sticking together, is another area we investigate. We're measuring cutthrough; the philosophy here is to compare the wires with the cutthrough characteristics of something as well known as Teflon. Thermal creep is an example of a slower form of cutthrough. In regard to wicking, we use fluorescent dye solutions to determine the extent to which a solution will wick up between the insulation and the conductor.

Then, as I say, we also expose these wires to ultraviolet radiation, both in vacuum and pure oxygen. We're doing the same thing with x-ray radiation. Flammability tests are conducted at 5 psi in pure oxygen. We're doing three types of flammability tests; one to simulate a short circuit condition, in which we put a slug of current through the conductor; the second is a rather slow increase of a current in the conductor: and the third type is with external heating, using a coil around the conductor.

We're working, remember, with only one wire size, and this is an unshielded, unjacketed type of hookup wire; we have been able to get most of these wires to burn in 5 psi pure oxygen. Other wire sizes were used in just a few preliminary tests. We have gotten fires with every kind of wire, including those of the H-film constructions.

Chemical compatibility tests are conducted with some seventeen different chemicals, including the various fuels and oxidizing agents, such as fluorine and N_2O_4; we expose wires to the chemicals and then use the flex test and the breakdown test to detect degradation. Of course, visual observations are also made.

Then we have offgassing in 5-psi oxygen. Here we are making weight-loss measurements using a quartz spring balance technique and gas analysis, determining what the products are. Then we conduct, in vacuum, volatility tests, in which we measure the products that come off and also measure weight changes.

From what I've heard here, I think that much of the work that we're doing at 5 psi is going to have to be done someday in the critical pressure range. I don't think it has been done yet. I think we're going to have to do more work in higher oxygen pressures. To my knowledge, I don't think a program this comprehensive has been conducted before. We've already seen from some of the results that one might expect different results from different wire sizes; shielded cables don't necessarily behave like a single wire, and a bundle of wires doesn't necessarily behave like a single wire.

I think there's a great deal to be done in this area, and I think some of the problems that have been discussed here might just be associated with hookup wire performance in the environments that you're exposed them to.

OPEN DISCUSSION

MR. BROWN: I'd like a little clarification. Did I understand you to say that you found that all the wires would support combustion? Did I understand you correctly?

MR. FRISCO: That's correct.

MR. BROWN: Even Teflon?

MR. FRISCO: Yes. With Teflon you can get a nice blue flame; the silicones too.

MR. BROWN: Well, what about polyolefin and, I believe, polyethylene?

MR. FRISCO: They're the best burners of them all. I think that is well known. As a matter of fact, we had an accident one day with a polyolefin wire during purging of the system. We accidentally lit the spark plug and the wire went up with no heat being applied to it at all. So, it's not a good actor at all in a flammability test.

MR. BROWN: Your work is being done with MSC?

MR. FRISCO: That's right.

MR. BROWN: Do they have any of your preliminary results yet?

MR. FRISCO: We sent a 259-page report to R. L. Johnson and Tony Wardell of MSC.

MR. BROWN: Okay; I know your source. I know where the information came from now.

MR. FRISCO: In fact, we were in Houston on Monday and Tuesday discussing it with them.

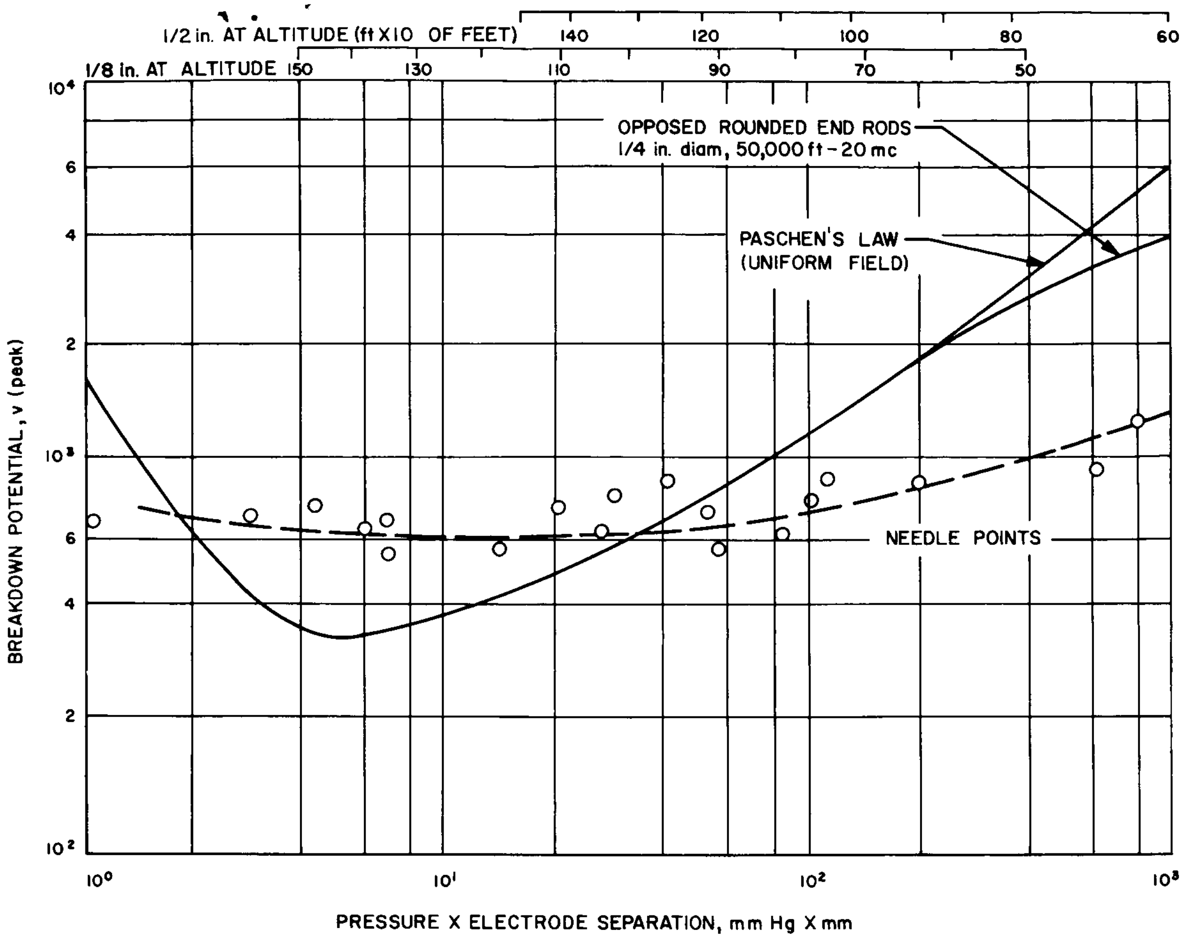

Fig. 19-1. Comparison of uniform field breakdown through critical pressure range

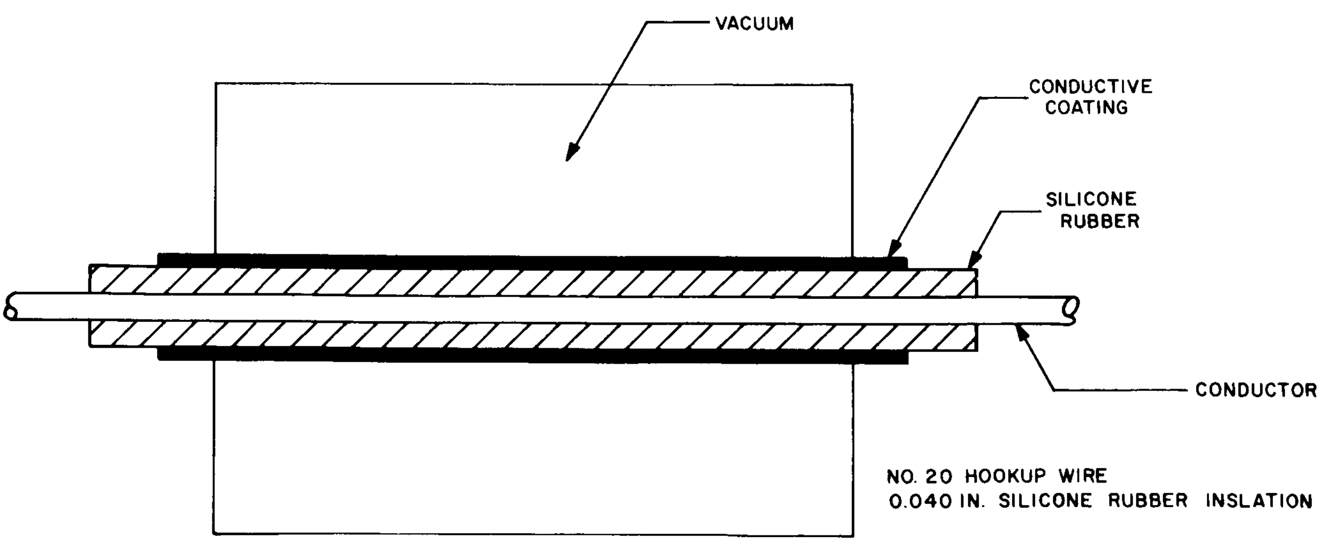

Fig. 19-2. Schematic of hookup wire sample